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AUTHOR Smith, Marshall S.  
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## ABSTRACT

The author discusses a need for planning research in reading, since present research seems fragmented, the federal government has not functioned especially well in the area of planning for funding or coordination with professional organizations, and there is a need to attract scientists in a variety of disciplines to work in educational research. The development of a research and development agenda in reading at the National Institute of Education (NIE) started in 1973. Meetings and conferences have been held by a group called the Essential Skills program, which was organized to plan a long-term agenda for reading research and development for NIE. A human information processing model for reading research which will draw on the knowledge of linguists, sociolinguists, logicians, computer scientists, psychologists, and others is the most frequently mentioned methodology suggested by participants in these programs. (MKM)

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Planning Research on Learning  
An example in Reading<sup>1</sup>  
Marshall S. Smith, NIE

U.S. DEPARTMENT OF HEALTH,  
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Introduction

I will discuss a planning process somewhat similar to that just discussed by Dr. McDaniels and Professor Gage. As in the case of the planning for an agenda in research on teaching our planning for reading was motivated by a number of concerns regarding prior research and development in the field. Specifically, four concerns stood out:

- 1) Research in the field of reading appeared to be fragmented or non-cumulative.
- 2) The Federal Government seemed to lack the ability to make constructive use of the state of knowledge in the field for funding new research and development.
- 3) There appeared to be a lack of positive and firm co-ordination between Federal funding strategies and the professional research and practitioner organizations around the country.
- 4) We felt a need to attract scientists in a variety of disciplines to work in the applied areas of educational research.

Unlike the planning for teaching research, however, the topic of reading research has a problem related focus -- a focus which allows the possibility of relevant and coordinated research to be carried out on characteristics of the learner, the materials and the instructional process. And unlike teaching research the methods for determining the applied success of the research -- effects on student learning -- are intimately tied to the problem. Adequate tests of student achievement in reading depend upon an understanding of the nature of the reading task. My sense is that these differences should make the task of creating a viable research and development agenda in reading more tractable than the creation of a similar agenda in teaching research. For if we imagine a prototypic instructional paradigm to rest upon at least four sources of knowledge; an understanding of the content to be taught; adequate instructional strategies; information about the developmental capabilities of the learners; and a picture of a mature learner (e.g., a competent reader); -- then reading research forces exploration of each of the areas in a manner specific to reading while the task in the teaching approach is to develop instructional theories or approaches which are generalizable to all content areas.

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<sup>1</sup>This paper was presented at the 1975 national conference of the American Educational Research Association on March 31st, 1974 as part of a symposium on NIE and Planning in the Social Sciences chaired by Arthur Wise. Other papers in the symposium were presented by Nathaniel Gage, "Research and Teaching" and Garry McDaniels, "Funding Non-paradigmatic Research". The opinions in the paper are my own and in no way represent the policy of NIE or any other part of the Federal government.

Let me give two examples of this point. First, we hear a lot about aptitude -- treatment interactions when instructional theory is discussed. It would be nice if we could come up with some generalizable rules about the relationships between student aptitudes and types of treatments. But it is hard to imagine that most "aptitude-treatment" interactions are not dependent upon the nature of the content being taught. After all the nature of the treatment is generally largely determined by the content and by the cognitive demands and the amount of prior information a student brings to the learning situation. Second, a favorite topic for many researchers in teaching is the effect of class size on achievement. Yet the study of class size in general -- in the absence of knowledge of the nature of the content, of the instructional goals and of the knowledge of the students in a particular content area -- seems foolish. For some purposes a large class size is appropriate -- for others one to one instruction may be most efficient.

There are not meant to be points of disagreement with Dr's Gage and McDaniels for my sense is that they generally agree with this argument. In the long run, of course, when we have a comprehensive and valid theory of instruction such concerns will not be important -- such a theory will take into consideration the nature of the content. In the meantime we may be able to develop content specific instructional theories based on evidence that interrelates all four aspects of the learning paradigm mentioned above. Fostering this later approach was our task in developing the agenda for reading research.

#### Toward an Agenda for Reading:

The development of an R&D agenda in reading at NIE started in the summer of 1973 when a small group of scientists headed by Professor George Miller of Rockefeller University came together to discuss issues in language and reading. The report which emanated from that meeting was presented to the National Council for Educational Research (NIE's Policy Making Body) in December of 1973. At that meeting the Council ratified five Institute priorities -- research in reading was one of the five. A small group in NIE called the Essential Skills program was organized to plan a long-term agenda for reading R&D for the Institute. During the spring and summer of 1974 a number of planning meetings were held and a larger number of planning documents were generated setting out directions for the Essential Skills program. Time does not permit a discussion of all these meetings so I want to summarize some of the critical things we have learned and focus on the major outcomes of the largest of the meetings -- a ten panel meeting similar in format to that described by Dr. Gage.

Turning to what we have learned from the conferences.

- 1) First, our expectations about the lack of cumulative effect of research in the field of reading was largely confirmed. Good reviews of the literature are hard to find. Syntheses of findings directed at the resolution of particular points of debate within the field are almost non-existent. The Reading Research Quarterly devotes one issue a year to summarizing work in reading research

but the summary is neither critical nor synthetic. Yet lurking in the minds of lots of researchers in the area are the seeds of important syntheses--they often can be orally expressed but they take time to write and the field does not seem to reward the effort of writing them. In the "decoding" or "word recognition" area, for example, a number of researchers have argued to us that sufficient new information exists to warrant a new generation of early reading curriculum development activities. Work in perception, studies of memory load, studies about the necessary speed of "decoding" for the facilitation of comprehension, notions about the importance of materials which are designed to be both interesting and appropriate to the experiences of different kinds of readers have abounded over the past few years. Yet nowhere is the argument set out convincingly on paper. This is not to say that there does not continue to exist considerable controversy over beginning reading. Systematic phonics instruction is not universally hailed. The sight or whole word approach in practically its purest form is firmly advocated by some leading psychologists and reading researchers. What it does suggest is that there is a great need for a number of major researchers in the field to pull back from their own private work every few years and give themselves the task of discovering in writing the distance between the state of knowledge and the state of practice. Perhaps the model for the syntheses should be the uninvolved scientist carefully capturing the essence of the field or perhaps the model should be one of advocacy where two or more researchers set out their biases before hand and attempt to confirm them in a scholarly manner. My own preference is that both (and other) strategies should be used. Whatever the model, however, the need is great.

2. Second, my sense from the planning exercise is that there are enormous untapped human and knowledge resources in the areas of cognitive psychology, linguistics, computer science, anthropology etc., that have a great deal to offer to reading research if given the incentives and opportunity. More on this later.
3. Third, I believe that we were naive at the beginning of our planning about the ease with which the Federal government might coordinate actual research in the field. Directed funding of specific basic research appears to be damned near impossible. General areas of exploration can be determined but the details of the studies -- specific hypotheses, methodologies and measurements seem to be best left to the scientists in the field. Specific policy work, on the other hand, can usefully be specified by Federal officials -- if only because people in the government are closer to many of the pressing policy issues. On the more programmatic issue of whether or not to concentrate limited research funds in only a few locations to attempt to insure a coordinated effort or to allocate the funds to individuals around the country to insure that the "best" possible ideas are tested, I come out to be an agnostic. There are good arguments on both sides of the issue.

- 4) Fourth, perhaps the major thing I have learned over the past year is that most researchers and practitioners in the field generally are not adept at generating research agendas. Although the format of our planning exercises may have inhibited some people, my sense is that the issue is deeper -- most researchers do not move gracefully into the role of long-range program planners. Their skills and expertise in experimentation does not seem to naturally generalize into the larger context. My generalization, of course, does not apply to all of the participants and it certainly does not mean that most of them could not become adept at the task given more experience -- I only suggest that the task is foreign enough not to be taken for granted. The condition of difficulty in planning seems particularly to exist in areas where there is either weak or non-existent theory. Sadly this seems to be the case in applied areas to a greater extent than in the more basic research field. This brings me to the large conference held over last summer.

Involved were some 50 participants and some 150 other advisors who generated papers for the conference. The fifty participants were divided into ten panels-- each focusing on an area related to reading. Six of the panels were asked to concentrate on the issues involved in reading comprehension. The panels ranged from proposing linguistic and cognitive psychological research on connected discourse to issues of the assessment of reading comprehension. Somewhat to our surprise a rough consensus appeared to emerge from panels focusing on reading comprehension. While the different panels proposed a variety of research projects there was a strong reliance on that methodology and knowledge generally called "human information processing".

The term "information processing" has been applied to a body of theory and knowledge which draws its conceptual basis from an understanding of the ways that people acquire, store, process and produce information. Combined with the work of anthropologists, a number of cognitive psychologists are beginning to have a serious understanding of the impact of cultural differences on the achievement of youngsters in schools. The work of computer artificial intelligence people and linguists has begun to give insights into the short-range contextual impacts of different kinds of semantic and syntactic structures. Put together with these insights is the work of a variety of psychologists and reading experts on differential strategies for attacking and comprehending different kinds of materials, on the use of structured questions for setting up expectations about the material to be comprehended, on the power of active rather than passive reading strategies, on the importance of technical vocabulary for comprehension of materials and on strategies for making materials more comprehensible.

One panel generally expressed their thoughts in the following way:

"We assume that current attempts to teach comprehension skills have not been as successful as hoped because they have not been based on a valid description of what is to be taught. At the present time, however, an active surge of research and theory construction has begun in the fields of psychology and artificial intelligence that is directly concerned with the structure and processes of any system able to understand and produce language. The 'state of the art' in these fields is developing rapidly."

Another panel, independently of the first, discussed information processing in the following manner:

"While much of what we are recommending retains a basic research orientation, we recognize the potential application of each of these approaches to the educational and communication process. If we know the basic goals, intentions and concepts which are to be communicated, and if we have procedures for using them to generate speech or written discourse, it may be possible to use these processes to generate coherent, structured texts that are readily understood, of interest, and which communicate the information accurately and efficiently. We would know the content and structure of our written materials. If we understood the details of the processes by which the material is understood, we could locate problems or failures to comprehend, and design training procedures to build skills that lead to more efficient processing. Knowledge of the context in which a message occurs may also aid in its transmission, reception and comprehension. Differences in cultural experience may aid or hinder and methods may be found to create a shared knowledge basis or transfer of language skills from one social situation to another."

While the research recommended is admittedly ambitious, it is still realizable. There exists through the recent efforts of linguists, philosophers, sociolinguists, logicians, computer scientists and psychologists a body of knowledge, skills and methods which can combine in various ways on each or several of the stages. This highly interdisciplinary character of the research attests to the complexity of the problem but it also testifies for its excitement in generating new knowledge and understanding."

When specific instructional strategies derived from the information processing approaches have been suggested to teachers it turns out that they co-incide with many of their intuitions. The congruence of the independent development of theory and the insights of successful practitioners seems to have created a rare situation in educational research -- a situation where we have some promise in advance that our research will have important long range effects. Part of NIE's focus over the next few years will be in attempting to pay off this promise.